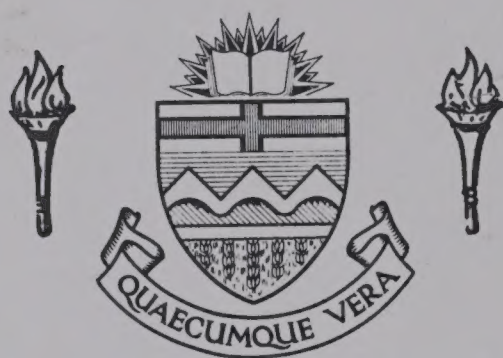


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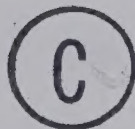
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ON THE INTERPRETATION OF GENERATIVE GRAMMARS

BY



PETER RUDOLPH HARRIS

A THESIS

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This thesis is concerned with the problem of interpretation and the problem of justification of that interpretation as these arise in the generative approach to language. In particular it

The undersigned certify that they have read, and recommend to the Faculty of Graduate Studies for acceptance, a thesis entitled "On the Interpretation of Generative Grammars" submitted by Peter Rudolph Harris in partial fulfilment of the requirements for the the degree of Master of Science. on of it as a prerequisite

ABSTRACT

This thesis is concerned with the problem of interpretation and the problem of justification of that interpretation as these arise in the generative approach to language. In particular it is argued that difficulties with justification stem chiefly from the fundamental obscurity of the current general interpretation in terms of the native speaker's competence, and further, that conceptual clarity at this basic level is not enough. The logical incoherence of the theory forces radical reconstruction of it as a prerequisite to serious consideration of it as a theory of language.

First some approaches to criticism of the generative position are examined and one adopted, and then some basic obscurities are revealed in the position. Following this it is argued that psycholinguistic data concerning a performance model must be considered in order to solve the problem of justification, and then it is shown that this cannot succeed because grammars cannot be linked to psychological events. On this basis it is claimed that the whole approach is misguided in terms of its excessive formalism, and nonrigorous, speculative character.

ACKNOWLEDGMENTS

I cannot in all truthfulness say it has been fun but I think it has been more than the usual trip (somehow broadening but of doubtful relevance) for me. If so I must say that it would not have been either possible or useful but for Professor C.I.J.M. Stuart. My thanks go also to Dr. Gary Prideaux, Dr. Bruce Derwing, and many learned strangers. Finally, I thank Julie Werenka - friend and typist. Rest easy Sylvester.

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§1: INTRODUCTION

All modern work in linguistics is of a truly monumental obscurity.

-- Jon Wheateley (1967)

In nineteen forty-five, Ernst Cassirer pointed out the need for a "logic of linguistics". Professor Wheateley's singularly unflattering remark comes almost a quarter of a century later. Whatever linguists may think of philosophical intrusions into their discipline, it seems clear that as long as the former's observation is ignored, the latter's will remain true.

We can consider the philosophy of science as comprising two components: (1) the logical analysis of the concepts and methods of science, and (2) the use of science to illuminate philosophical problems (Smart 1968). The first one only is relevant here. However it is possible to view this type of activity in at least two ways. I am not interested in a philosophical account of linguistics as a contribution to the philosophy of science, but rather in the use of philosophical techniques and concepts in clarifying the nature of linguistic inquiry, as a contribution to theoretical linguistics.

The ultimate purpose of inquiry is to provide scientifically effective explanations of the nature of language. In the performance of this task, however, the scientist must be clear as to the exact nature of the immediate purpose of inquiry, and he must also be assured as to the logical coherence of his apparatus (Stuart 1964).

Despite their rather obvious self-consciousness about the scientific status of their discipline, modern linguists have for the most part neglected to inquire into the foundations of linguistics. This is not to say that no work has been done

and nothing accomplished; such an assertion would be false. As Kuhn (1962) has pointed out, periods of paradigm-shift are characterized by debates over the legitimacy of competing sets of methods, problems, and standards of solution - the work of Bloomfield and Chomsky contain the most outstanding examples of this. However, it is my contention that the obscurity referred to above is not simply the result of a paradigm shift but rather is fundamental to the whole of structural linguistics, in the more general sense of "structural". Notwithstanding the polemics, the problem remains: How are we to understand what it is we are doing?

The need for such an investigation is particularly pressing at this time in linguistics because of the tremendous widening of both the influence and scope of the discipline within the last decade or so.¹ The study of the biological foundations of language is well under way and psycholinguistics has emerged as a major branch of contemporary psychology. In addition, philosophy has been advised to make use of the results of so-called "empirical linguistics".² The point is that we must now recognize non-linguistic constraints on our activities, and we are faced with criticisms where we were previously ignored.

The motivations for foundational studies are thus several. If we wish to look upon ourselves as being scientists (and it seems that we do) and if we expect workers in other fields to accept our analyses (and we do), then we must develop the prerequisites for an evaluation of the scientific status of linguistics. This sort of goal necessarily implies another -

the introduction of a reasonable level of theoretical and methodological coherence into our discipline. Achievement of such aims will involve sustained investigations which

... lie at the intersection of linguistic theory and general epistemology. In particular, they are concerned with the foundations of linguistic theory and lead, for example, to questions as to what kinds of knowledge are established in linguistics, what meanings and interpretations may be given to linguistic formulations, and what meanings and interpretations are excluded. ... One way of approaching such issues is in terms of rather basic experiential modalities. Under this heading I would include the empirical, conceptual, logical, and operational ... foundations of linguistic experience and hence, of linguistic theory. ... In this sense, the empirical, conceptual, logical, and operational modalities form the experiential conditions of knowing and meaning in science. What these conditions are in linguistics, what kinds of knowledge they establish, and in what ways they restrict the meaning of linguistic formulations, seems to me to be quite obscure (Stuart 1969).

This essay is more restricted in purpose. It will be concerned only with selected difficulties in generative-transformational approaches to language. Any attempts at generalization beyond the domain of this rather piece-meal effort, must, for the present, remain more suggestive than substantive. My goal is that of explication and of theory reconstruction through critical examination and assessment of the assumptions, methods, and concepts underlying this generative paradigm. In this paper I can do little more than show the need for such an undertaking and indicate the direction it seems to me it should take.

1.1. The new paradigm

The decade just elapsed has been witness to the "Chomskyan revolution" in linguistics. A comprehensive account of these developments in their historical context has yet to be written

and would almost certainly be premature at this time. Indeed, one may wish to question whether the label "revolution", in Kuhn's sense, is correctly applied in this instance. This sort of terminological squabble need not be settled here; perhaps, after all, revolution is in the eye of the beholder. Not only have the effects of Noam Chomsky's³ ideas been felt in every branch of contemporary linguistics, but they have also had great influence in related disciplines. Certainly the adherents to the Chomskyan position⁴ believe they are part of a revolution.

Many observers have been rather dismayed by the style of the transformationalists; particularly by what the Voegelins (1963) refer to as Chomsky's "eclipsing stance", by the polemical rhetoric, the one-sided historicizing, and all-round "unscientific" behavior. In addition to being in essential agreement with many of Paul Feyerabend's (1963) comments on these issues, I would underline the fact that they are true of the natural sciences (as opposed to the "younger" social or behavioral sciences) more often than some would have us believe. Thus I have no particular qualms about this sort of propagandizing, up to a point. That point is reached when it becomes clear that these very influential promissory notes cannot in fact be honored by subsequent demonstration.

There seem to be two attitudes current among those linguists who accept the generative framework. The first is characteristic of those among Chomsky's partisans who passively accept his ideas about linguistics and psychology and their interrelationship. Their position is at least understandable.

The second attitude is that of people like Lyons (1966) who remarks, in a review of Chomsky's Aspects, that there is "a good deal of irrelevant and tendentious argument about 'mentalism', 'innate ideas', and 'intuition'." Tendentious it certainly is, but I would hardly consider it irrelevant unless that simply implies leading to confusion rather than enlightenment. For it is just these concepts that form (along with others like "competence") the very basis of the attempt to make generative grammar something more than a purely formal exercise. Thus they are part of the theory's foundations, and foundations are simply not superfluous.

What I am advocating is an attitude which differs from both of those mentioned above and differs also from that of linguists of other theoretical persuasions, which tends to be excessively polemical rather than contributive, and to involve essentially destructive criticism. In other words, I am calling for an approach which neither uncritically accepts what is offered, nor misses the point, nor uncritically rejects. I do not happen to believe that linguistics is overburdened with interesting and valuable theories, therefore I am reluctant to dismiss Chomsky's ideas as long as I see no viable alternatives.

I should perhaps re-emphasize here my belief that Chomsky has led our discipline in new directions, many of which are entirely salutary and constitute solid advances. This in no way contradicts my equally strong affirmation of fundamental and perhaps unresolvable difficulties underlying the whole approach. The problem lies in disentangling these two

strands in a principled manner.

1.2. Scope and objectives

Generative grammar has been extremely influential and not notably subject to sustained critical (i.e. analytical) appraisal.⁵ Its proponents make strong claims for the attention of, and acceptance by, the scientific community in terms of

- (1) the introduction of a thorough-going hypothetico-deductive method, and
- (2) the extension of the domain or range of linguistic theory.

There are three major features with respect to (1):

- (1.a) the shift in interest from the context of discovery to the context of justification.
- (1.b) the claim that grammars are theories in the good scientific sense of the term "theory".
- (1.c) the claim to go beyond description to explanation.

Factors in (2) above include:

- (2.a) the explicit recognition that a linguistic theory is a psychological theory if it is any kind of a theory at all, and thus that
- (2.b) data from the investigation of normal language functions, language ontogeny and speech pathology are desiderata, and, more generally,
- (2.c) linguistic theories are required to be compatible with psychological theories about other human abilities and consistent with neurophysiological knowledge.

My objectives are:

- (A) to examine the generative position in terms of its internal conceptual and logical coherence (rather than in terms of natural language data) and, consequently, the extent to which the claims above are justified.
- (B) further, to establish specific conclusions with respect to (1) and (2) above.

This essay is not intended as a contribution to the so-called "Great Generative Controversy" (Starosta 1969), nor is it to be construed as an attack on the Standard Theory in favor of one of the proposed variants of it. My conclusions will be used to suggest that linguistic methodology needs to be reformulated in the direction of greater biological realism and a central role for experimental investigation.

1.3. Statement of thesis

Chomsky's theory is conceptually obscure and logically deficient to such an extent that it is both untestable and explanatorily inadequate.

1.4. General approach

I will be dealing chiefly with Chomsky's view as presented in the first chapter of Aspects of the Theory of Syntax. Chomsky describes this chapter as a brief survey of some of his main background assumptions; he is making no serious attempt to justify them, only to sketch them clearly (1965: 3). I claim that he has done neither, and that his subsequent statements have not significantly altered the situation.

The difficulties I find in Chomsky's position are of two main sorts: (1) conceptual - notions like "competence", "performance", and "intuition" demand explication, and (2) logical - there is what might be called an inferential gap between linguistic formalisms and observable events; indeed, the logical apparatus necessary to bridge this gap is not part of the theoretical structure as such. In the light of these considerations I shall examine the competence/performance distinction and the problem of justification.

Chomsky usually (and quite justifiably, in most cases) charges his critics with misrepresentation and/or missing the point. Insofar as fundamental obscurities and systematic ambiguities will allow, I have tried to present his views clearly and, I hope, fairly. For this reason I have made extensive use of direct quotations, and, more importantly, used Chomsky's own arguments as much as possible in making my points.

FOOTNOTES

1. (p.2) "Modern linguistics is perhaps second only to philosophy in its interdisciplinary ramifications" (Smart 1968: 327).
2. (p.2) This position has been put forth most strongly by Fodor and Katz (1964) and Katz (1966). Katz's arguments seem to have been almost generally rejected - see, e.g., Moravcsik (1967). For Chomsky's views on this, see 1969a.
3. (p.4) The Term "revolution" may raise difficulties in this context, but "Chomskyan" is absolutely appropriate.
4. (p.4) It should be clear that this is not intended in the narrow sense where Chomsky's position is opposed to, e.g., case grammar or generative semantics.
5. (p.6) The latter part of this assertion needs qualifying in certain respects; more especially, to reiterate somewhat: (a) most such criticism has been relatively superficial, and (b) those valuable critiques extant have attracted little attention and serious consideration. Much the same can be said of previous and coexisting positions.

§2: ON CHOMSKY CRITICISM

2.1. The new mentalism

The generative approach to linguistics represents an attempt to specify what is meant in saying that someone has learned a language, or that someone knows a language. More specifically, it aims at characterizing what is learned, the knowledge that a native speaker has of his language which enables him to produce and interpret indefinitely many sentences in it.

We thus make a fundamental distinction between competence (the speaker-hearer's knowledge of his language) and performance (the actual use of language in concrete situations). (Chomsky 1965: 4)

For Chomsky, competence is the subject matter of linguistics (in fact it must be if this is to be a serious discipline) and competence is a psychological entity - "a mental reality" (1965: 4). Linguistics is thus in one way pushed back into the Saussurean tradition, the descriptivist position being regarded as regrettably aberrant in this respect (see e.g. Levin 1965). More importantly, a linguistic theory is to be considered as being in some sense a psychological theory, and the psychological framework is clearly "mentalistic" - linguistics is a branch of cognitive psychology (Chomsky 1968: 1). (In this context "cognitive psychology" and "psychology of cognition" are not synonymous, the former referring not only to the subject matter involved but also to the theoretical positions of researchers who oppose "cognitive" to "behavioristic").¹

I call it an approach to linguistics in order to indicate its fundamentally monistic character. Language behavior displays competence and language behavior is rule-governed behavior par excellence. Thus it is assumed that the native speaker has "mastered and internalized a generative grammar that expresses his knowledge of his language" (Chomsky 1965: 8). The task of the linguist becomes one of attempting to describe or model this intrinsic grammar (knowledge of a system of rules, competence of the native speaker). At this point one can raise an ontological problem: how far does Chomsky's professed Cartesianism go? At any rate a definite commitment has been made - linguistic descriptions (grammars) are to be interpreted in a definite space, namely the mind of the speaker-hearer. That is, linguistic theory is concerned to discover "a mental reality underlying actual behavior" (1965: 4).

Chomsky's setting of this goal for linguistic theory has not gone unchallenged. Leonard Bloomfield (1933) warned linguists to maintain their independence of psychology in order to safeguard their results and make them more significant to workers in other fields. He was mainly concerned that linguistics would otherwise be continually undetermined by shifts in psychological doctrine (speaking from his own experience with his 1914 Introduction to the Study of Language). However, he in fact adopted a behaviorist ("mechanist") position both as being more scientific and as being safer and more solid than mentalism. Chomsky, on the other hand, does

not appear to believe in such an artificial separation of disciplines. He has attacked both Bloomfieldian linguistics and behaviorist psychology on behalf of what he considers a much less impoverished view of science (the hypothetico-deductive method) and of the linguistic and psychological enterprises (mentalism). The concept of a generative grammar is to be taken quite literally - it is not simply a revealing formalization to capture regularities discernible in a corpus (whether actual, or intuitively possible) - it refers to the underlying system of psychological processes.² Chomsky is not interested in a safe taxonomy but in explaining human language and ultimately, via this understanding, in approaching the human essence (the difference in man and the difference it makes - what governs the rules?).³ However, the Bloomfieldian position is still being used to attack Chomsky (e.g. Esper 1969).

2.2. Two critics

Adopting what Esper likes to think is a "more wholesome attitude of modesty and sobriety", we can bewail Chomsky's ontological seduction, his rationalism, his neo-medievalism; but regardless of how we decide on the question of scientific progress (with Kuhn or with Popper), I do not believe that Esper is justified in describing Chomsky's marching orders for linguistics as leading in a U-turn back to seventeenth-century metaphysics (of course in a sense Esper is obviously correct, but I am denying that his term "retrogression" is called for - more on this below). He attacks Chomsky as

being a mentalist, a rationalist, a nativist, and an intuitionist. Presumably someone like Katz would dismiss Esper as an operationalist, a behaviorist, a physicalist, and an empiricist (cf. Katz 1964). Neither is being very helpful.

Garvin (1970), who advocates a "moderate" ("more whole-some"?) theoretical position, uses some of Abraham Kaplan's ideas in support of a somewhat different kind of attack - this time on Chomsky's "extremism" (and Bloomfield's as well). Garvin mentions "the well-known older controversy over mechanism versus mentalism, which has been recently rekindled under the heading of rationalism versus empiricism". (This is a confused and confusing way of putting it, but I will ignore that). Garvin goes on to quote Kaplan as follows:

There is a story of a drunkard searching under a street lamp for his house key, which he had dropped some distance away. Asked why he didn't look where he dropped it, he replied, "It's lighter here!" Much effort, not only in the logic of behavioral science, but also in behavioral science itself, is vitiated, in my opinion, by the principle of the drunkard's search (Kaplan 1964: 11).

The pressures of fad and fashion are as great in science, for all its logic, as in other areas of culture ...

In addition to social pressure from the scientific community there is also at work a very human trait of individual scientists. I call it the law of the instrument, and it may be formulated as follows: Give a small boy a hammer, and he will find that everything he encounters needs pounding. It comes as no particular surprise to discover that a scientist formulates problems in a way which requires for their solution just those techniques in which he himself is especially skilled (1964: 28).

Garvin's use of a philosopher of science to legitimize his position seems reasonable and acceptable, but he should acknowledge his selectivity because Kaplan goes on to say:

But the joke may be on us. It may be sensible to look first in an unlikely place just because "it's lighter there". We might reasonably entertain one hypothesis rather than another because it is easier to refute if false, or because it will eliminate a greater number of possibilities, or because it will show us more clearly what steps to take next (1964: 17).

And further:

The law of the instrument, however, is by no means wholly pernicious in its working. What else is a man to do when he has an idea, Pierce asks, but ride it as hard as he can, and leave it to others to hold it back within proper limits? What is objectionable is not that some techniques are pushed to the utmost, but that others, in consequence, are denied the name of science (1964: 28).

Thus Kaplan's notions can be used in a defence of the generative approach. For example, consider his "law of the instrument". Given only the first quotation from Kaplan on the issue, it would seem that there is indeed something childish about all this. After all, Joos (1957) says "Children want explanations, and there is a child in each of us; descriptivism makes a virtue of not pampering that child". But given Kaplan's added qualification, I think one could draw an analogy with a point made by Marcuse (1966): "In and against the deadly efficient organization of the affluent society, not only radical protest, but even the attempt to formulate, to articulate, to give word to protest assumes a childlike, ridiculous immaturity". If this sort of thing seems more a case of rationalization than rationality, one can answer both Garvin's moderation and Esper's behaviorism in more respectable (and more useful) terms.

In order to do this I will now develop a concept of theoretical depth. I will use it to comment on the kinds of

criticism put forth by people like Garvin and Esper, and then later to develop my own critique of Chomsky.⁴

2.3. Theoretical depth

Theories can be classified on the basis of depth as phenomenological or as representational (these are extremes, in between are semiphenomenological or semirepresentational theories). Phenomenological theories (less deep) are black box theories which can be characterized by an equation relating the input I and the output O:

$$O = M I$$

where M (a variable, operator, or function) mediates or intervenes between the external variables I and O. In a black box theory the mediating symbol "M" epitomizes the properties of the box, but M is not derived on the basis of such properties. It is an uninterpreted (or intervening) variable, a purely syntactic link between a column of values for the input I and a column of values for the output O.

In a corresponding translucent box (representational) theory, M becomes an interpreted variable (hypothetical construct). That is, M becomes a complex symbol or derivative variable concerning the structure and operation of the box. It is assigned a referent, a set of internal variables characterizing the properties of the box - it represents the "mechanism" which mediates between the input and output. The greater depth of such a theory depends on these characteristics: the presence of high-level constructs, the postulation of a definite mechanism, and a high explanatory power. More

formally, a theory T_1 is deeper than a theory T_2 if and only if: (a) T_1 incorporates higher-level constructs (unobservables) than does T_2 ; (b) these constructs occur in hypothetical mechanisms underlying the data referred to by T_2 ; and (c) T_1 logically explains T_2 . This definition points to three closely-linked aspects (respectively: epistemological, ontological, and logical) of any deep theory. Only by introducing transempirical concepts can we posit unobservable mechanisms, and only what we hypothesize as occurring deep inside the box can explain those things observed at the surface.

Phenomenological theories are consistent with any number of specific mechanisms; they are wholistic in character and safe. In addition they are epistemologically simple in their avoidance of nonobservational concepts, and accurate in that the parameters can be arbitrarily adjusted. Hence they are widely employed and not only by those with a distrust of theory. However, they are at the same time less complete and less definite than corresponding representational theories; they take few chances, and they have a low heuristic power (in terms of deep problems). In other words, deep theories are more specific and hence more informative. As a consequence of their greater definiteness or commitment they are more testable, and being more testable they are more apt both to lose and to acquire a good empirical foundation. The set of relevant data is more varied and thus there is a greater chance of unfavorable evidence emerging. In brief, deeper

theories are more falsifiable; and I contend (with Popper) that this is a requirement essential to the growth of scientific knowledge.

In general, the depth of a theory is tied to the depth of the problems it attempts to solve: the deeper the problems it solves successfully, the deeper the theory. The historical trend in the sciences has been that of supplementing black boxes with translucent boxes, of going beyond the superficial in search of explanations. Of course these so-called translucent boxes are actually collections of smaller black boxes, given our present state of knowledge, but this should not obscure the point. An input-output relation poses the problem of theorizing about possible mechanisms to account for it. One can certainly decide not to tackle such a problem, but he is not necessarily thus entitled to discourage others from making the attempt or to condemn them in the name of some philosophy (black-boxism), in this case one which would entail abandoning much of modern science. On this point I tend to stand with Kaplan - norms or standards of scientific inquiry ought not to be derived from something outside of science. Hence I remain suspicious of any attempt to impose a certain philosophy of science on linguistics, a la Bloomfield; indeed, given the present situation in metascientific studies, I don't know what it would be like. In summation, I think that Chomsky's ideas must be considered with respect to their potential as a deep theory, if only because he himself presents his position, and replies to critics in such terms.

2.4. A new approach needed

Chomsky's warrant for making deeper claims for grammars is his posited relationships among a grammar, a language acquisition device, and a production-perception model. A structuralist grammar could not even be considered in such relationships because of the lack of the concept of generation, and more specifically, the failure to distinguish between deep structure and surface structure, and to recognize the necessity of transformational rules. These terms ("generate", "transformation", "deep structure") immediately suggest things psychological, unless you are behaviorist or simply not interested in "psychologism". Interestingly enough, Chomsky attacked Bloomfieldian linguistics first (1957), then behaviorist psychology (1959), and only subsequently espoused "mentalism" (esp. 1964, 1965, 1966).

In the meantime the "cognitive" or "information-processing" approach was emerging in psychology to mount a major challenge to both radical behaviorism and the more recent mediation theory. Minsky speaks of "the intellectual revolution that came with the discovery that at least some mentalist descriptions of thought processes can be turned into specifications for the design of machines or, what is the same thing, the design of programs ... there exists no other, competing family of experiments from the positivistic, behavioristic tradition in psychology" (1968: 2).⁵ This approach is not limited to those drawing on work in artificial intelligence or making use on simulation as simplistic, although he makes use of

the program analogy. The common enemy is peripheralism based on the concept of the "empty organism". In general these approaches are "mentalistic" in making representational claims about complex central processes underlying surface behavior (they are pushing deeper). Chomsky has made it explicit that his linguistics is to be considered as a branch of cognitive psychology. The question remains as to what extent linguistics is working in the same sort of conceptual and methodological framework as other areas of cognitive psychology. Postal (1968), for example, uses the program analogy also. I do not think this implied integration works; however there are several general issues which impinge on this problem to be dealt with before I elaborate.

The first has already been mentioned - the ontological problem involved in Chomsky's "mentalism". Chomsky has provoked a lot of scorn on this point. Some critics (e.g. Hook: 167) have not hesitated to wonder out loud whether Chomsky has conjured up the old Cartesian ghost. I don't think anyone who has been reading Chomsky closely (e.g. 1965: 193) or is familiar with what some of his followers have been saying (e.g. Fodor 1968a) need concern themselves with this kind of charge. It is, however, typical of Chomsky to attract this kind of argument (the reference above is to a footnote). Whether or not he does this deliberately, it turns out to be useful for siphoning criticism into either simply dealt-with formulations or philosophical limbo.

I think it is reasonable to describe Chomsky's mentalism as "methodological" in the same sense as one can speak of a

"methodological" behaviorism, distinguished from the "philosophical" version. This is why Chomsky does not accept Bloomfield's dichotomy between "mentalism" and "mechanism". The difficulty had to be dealt with in recent psychology as well, with respect to modelling of internal processes:

The events postulated in such models are not "mentalistic" in the sense that they refer to some reified entelechy storing and processing data. Rather they are descriptions, at a particular level of abstraction, of postulated outcomes of neural activity (Forehand 1966: 381).

I think this applies to Chomsky's work as well. The ontological problem here shades into the epistemological problem - what are the conditions of knowing?⁶

There have also been many disparaging comments made about faddism in linguistics, with reference to the generativists (e.g. Garvin and Esper). Once again we can refer to a similar situation in cognitive psychology. Consider Forehand's assessment:

Does the new emphasis on cognitive processes signify psychology's long-promised involvement in the detailed study of complex, integrated, nonartificial human behavior? Or is it the excitement of the moment, feeding upon itself and building a rickety structure of poorly defined concepts and unsupportable assumptions? Is it renaissance or fad? Several signs encourage the renaissance interpretation. First, the major approaches have developed out of time-tested traditions of research and theory, inheriting the facts, methods and languages of their forebears. Secondly, the traditions are several; scholars working on diverse problems from diverse points of view find themselves led by the development of their research programs to ask similar questions. Thirdly, the resulting studies bear the earmarks of maturity: disciplined interplay among hypotheses, theory, method, and data (1966: 356).

Whether or not this kind of statement is true of psychology, my main point is that one could well imagine a generative

linguist saying the same sort of thing. This would be parallel to the kind of name-calling exchange I imagined between Katz and Esper, in terms of its uselessness. Considerations of this sort taken together with the implications of the concept of theoretical depth should be sufficient indication that Chomsky's position requires a critical rather than dogmatic kind of attack, and is also hardly likely to be affected by socio-historical observations in the name of "moderation".⁷

FOOTNOTES

1. (p.10) See Anderson and Ausebel 1965, p. vi.
Chomsky's remarks about recent psychology can be misleading insofar as he identifies behaviorism with Skinner, and insofar as he ignores well-established non-behaviorist approaches (cf. also papers in Kleinmuntz 1966).
2. (p.12) Chomsky here rejects Saussure's conception of langue as merely a systematic inventory of items, in favor of an Humboldtian conception of an underlying system of generative processes (1965: 4).
3. (p.12) Hilary Putnam (1967) remarks that it is the possibility of attaining such goals that gives linguistics its "sex appeal" for Chomsky.
4. (p.15) The concept of "theoretical depth" is borrowed from, and developed in a fashion heavily dependent on, Bunge 1967 I: 506-515). It should be noted that the terminology of 2.3 is Bunge's, and is in some respects idiosyncratic (esp. "phenomenological"), but his distinction between "hypothetical constructs" and "intervening variables" is essentially that of MacCorquodale and Meehl (1948).
I am using the idea of depth as a tool, hence there is a danger of adopting an arbitrary stance. There is also the possibility of thus introducing a bias in favor of the position I am arguing for. In this case I am forcing discussion in terms that Chomsky himself proposes in support of his approach.
5. (p.18) The general tone of Minsky's remarks is very reminiscent of Chomsky's Language and Mind. Herbert Simon observes:
Historically, the modern theory of transformational linguistics and the information-processing theory of cognition were born in the same matrix - the matrix of ideas produced by the development of the modern digital computer, and in the realization that, though the computer was embodied in hardware, its soul was a program. One of the initial professional papers on transformational linguistics and one of the initial professional papers on information-processing psychology were presented, the one after the other, at a meeting at M.I.T. in September 1956. [He refers to Chomsky's "Three Models for the Description of Language" and Newell and Simon's "The Logic Theory Machine"]. Thus the two bodies of theory have had cordial relations from an early date. And

quite rightly, for they rest conceptually on the same view of the human mind (1969: 47).

Chomsky, on the other hand, doesn't care for this kind of statement of historical association. He prefers to see the main impetus in his career as being his return to the concerns and doctrines of "traditional" linguistic theory, i.e. "Cartesian linguistics".

6. (p.20) I am not simply dismissing the problems involved with the status of these sorts of "mental mechanisms", but rather holding them in abeyance, as being not specific to linguistics. Indeed, it has been claimed that "the consequences of adopting the cybernetic view of psychology, theoretically or experimentally, have not yet been investigated" (Pask 1966: 138). But my main concern here is first of all with those difficulties peculiar to Chomsky's approach, and secondly with those which are more fundamental but still within the domain of linguistics.
7. (p.21) ... A sequence of recent publications has indicated that many points that I had hoped to take for granted are widely regarded as controversial, and has also indicated misunderstanding, on a rather substantial scale, of the general framework I had expected to presuppose - in particular, a misunderstanding as to which elements of this framework express substantive assumptions about the nature of language and are, therefore, matters of legitimate controversy and rational discussion, and which, on the other hand, relate to questions of goals and interests and are therefore no more subject to debate than the question: is chemistry right or wrong? (Chomsky 1966b: 1)

I tend to agree with this statement of Chomsky's as far as it goes. My point, however, is that there is another set of elements which is either missed entirely or not clearly separated from the rest (on the part of both Chomsky and his critics); namely, those of a general methodological or foundational nature. The question here is whether or not one's logical and conceptual apparatus is sufficiently powerful, or even of the right type, to justify claims about achievement of one's stated goals.

§3: A PRELIMINARY NOTE ON CHOMSKY

In my introductory remarks I indicated that my terms of reference for examining Chomsky's position would be conceptual and logical considerations rather than natural language data. In the second chapter I presented instances of approaches which do not command attention (except as negative examples) precisely because they fail to address themselves to these kinds of issues in any serious way. Fortunately not all Chomsky criticism is in such a state. Unfortunately, most of what useful discussion there has been is focussed on problems in the philosophy of mind which perhaps seem irrelevant to linguistics as such (recall Lyons' comment on Chomsky's discussion of innate ideas, p. 5). Rather than argue on the question of relevance here, I simply assert that Chomsky considers his work in these areas to be inseparable from his purely linguistic endeavors. Indeed, over the last few years his attention has been increasingly directed towards these problems.

In this chapter I briefly delineate two of these issues. I am not concerned so much with bringing these questions to the attention of linguists as with pointing out some things about the way in which Chomsky deals with them.

3.1. Language and rules

The concept of a "rule" is notoriously problematic in philosophy. The notion "rule of language" is perhaps even more so, there is even disagreement as to whether or not the word

"rule" is misused in this connection (e.g. Black 1949 versus Ziff 1960). In any case it does seem to be a somewhat idiosyncratic sense of the term which is involved. Granted that people do use or misuse the term in this context, what do they mean by it? A question like this leads to a lot of puzzles. I can only at this time point out that, given the fact that the concepts of rule-governed behavior and rule of language are absolutely central to the generative position, the lack of attention devoted to understanding these notions on the part of Chomsky and Katz is quite remarkable.

Chomsky proceeds by way of a basic assumption - that language is based on a system of rules. That is, speaking in a language is regarded as a form of rule - governed behavior. This does not (apparently) simply mean that regularities shown in performance can be described in terms of rules. Rather, speaking is a process somehow involving rules. This implies further that learning a language involves, among other things, acquiring a set of rules. One can of course raise the question as to whether there are, in fact, internalized rules.

Wiest (1966) has attacked Chomsky on just this question. He insists that a demonstration that a child has learned grammatical rules would be "his exhibiting the verbal performance called 'uttering the rules of grammar'" (p. 220). Chomsky dismisses this kind of statement as merely another instance of the "conceptual gap" he finds in contemporary psychology,

which refuses to consider the question of what is learned (1968: 85n).¹ Wiest's main point (which Chomsky seems to miss) is that claiming that the child has internalized the rules of grammar "involves a theoretical inference which conceivably might be useful but, as with all inferences, is not logically required" (p. 220). This raises an interesting problem. Chomsky says things like: "Obviously, every speaker of a language has mastered and internalized a generative grammar that expresses his knowledge of his language" (1965: 8); and "Clearly, a child who has learned a language has developed an internal representation of a system of rules that determine how sentences are to be formed, used, and understood" (p. 25). The argument underlying these assertions has never really been specified in detail. At any rate, it seems to be based on the necessity of accounting for the so-called "creative" aspect of language (cf. Katz 1966, Wheateley 1967). However, Chomsky has more recently undercut a lot of these difficulties by a methodological assumption:

We postulate that a speaker of a language has an unconscious knowledge of the rules of grammar if this postulate is empirically justified by the role it plays in explaining the facts of use [performance?] and understanding and acquisition of language (1969b: 155).

But what Wiest is getting at is that inferences about internal organization, while conceivably useful, can readily degenerate into a process of what he calls "pseudoexplanation by mere naming". Berlyne (in Kleinmuntz 1966: p. 347) makes the same sort of comment about the use of the terms like "rule" or "principle" in information-processing psychology. Now I think

that one can make a case against this point of view in terms of choice of an appropriate level of conceptualization for handling certain problems, to be judged by some sort of criteria of success. But in the case of the linguistic use of "rule" this defense is highly dubious. "The conception of 'rule' that is to be psychologically relevant as the basis of a psycholinguistic theory of performance must be something quite different from the linguistic one" (Jakobovits 1969).² If this can be accepted, then what happens to the "empirical justification" of Chomsky's postulate? Of course he may be basing his position on some esoteric sense of "explanation", in which case he should tell us what it is.

There is a more purely philosophical difficulty entangled in the problem of understanding "rule". Chomsky has forced the question as to whether it is misleading to speak of the native speaker's knowledge of innate principles and/or grammatical rules.

3.2. Competence and knowledge

Chomsky introduces the term "competence" with reference to the native speaker's knowledge of his language. Is competence to be identified with the native speaker's knowledge or to just some particular aspect of that knowledge? Or, alternatively, does our theory claim to describe the whole of competence?

This competence can be represented, to an as yet undetermined extent, as a system of rules that we can call the grammar of his language (1964a: 51).

Even more fundamentally, perhaps the term "knowledge" is a misleading one to use in this context.

Many philosophers, following Ryle, distinguish between the procedural and propositional senses of "know"; roughly, knowing how as opposed to knowing that. Many conceptual difficulties arise with interpretations of Chomsky in this respect. Is he talking about knowing how or knowing that? Is he trying to reduce knowing how to knowing that? (Can that be done?). Chomsky rejects the dichotomy out of hand (1969a: 87). He insists that he is talking about tacit knowledge (tacitly knowing that or knowing how, or something else?), but without further elucidation.³ Philosophers have very good reasons for wanting to be clear about the logical geography of various concepts, but the character of debates on this particular issue seem to preclude this.⁴ However, some important points can be gotten out of it.

Scheffler (1965: 92) states that knowing how to "represents the possession of a skill, a trained capacity, a competence, or a technique". Is this the sense of "knowledge" that Chomsky intends? It would appear not, given my remarks above. Harman (1967), for example, discusses competence in terms of knowing how and is rebuked by Chomsky, who says "there is no reason to suppose that knowledge of language can be characterized in terms of 'knowing how'" (1969a: 87). Chomsky also says:

In my sense of 'competence' the ability to speak and understand involves not only 'competence' (that is, mastery of the generative grammar of the language, tacit knowledge of the language), but also many other factors (1969a: 87).

Now earlier in the same article Chomsky differentiates between a presystematic and a systematic level of introduction of the notion of "competence".

At the presystematic level, I have tried to explain what I mean by 'linguistic competence' in terms of models of use and acquisition ... At the systematic level, competence is expressed by a generative grammar that recursively enumerates structural descriptions of sentences, each with its phonetic, syntactic, and semantic aspects (p. 81).

It is at the second (systematic) level that he develops the concept "in as precise a way as the state of the field permits" (p. 80). Of course he has not done this. In fact he has added to the confusion himself on a number of occasions despite pretensions of precision. For example, he has said that

the technical term 'competence' refers to the ability of the idealized speaker-hearer to associate sounds and meanings strictly in accordance with the rules of his language. (1967a: 398, my emphasis)

But consider again his reply to Harman:

In my sense of 'competence' the ability to speak and understand involves not only 'competence' (that is, mastery of the generative grammar of the language, tacit knowledge of the language), but also many other factors (1969: 87).

My point is that Chomsky himself gives no strong indication that he has pursued any deep analysis of notions critical to his position. When the question of justification is examined closely it turns out that the difficulties with it turn upon difficulties with these same foundational concepts.⁵

FOOTNOTES

1. (p.26) In the course of his reply to Wiest, Chomsky says that there are no behavioral regularities associated with (or in) the understanding and production of speech. I don't really know what he is trying to prove.
2. (p.27) I turn to the task of supporting this assessment below (§5).
3. (p.28) The only treatments of this that I am aware of are Fodor (1968b), and replies to Fodor by Nagel (1969b) and Morgenbesser (1969).
4. (p.28) See, for example, the Chomsky-Harman exchange (H 1967, C 1969a, H 1969, C 1969b). Arbin (1968) also joined in, and is replied to by Harman (1968).
5. (p.29) For discussion on the other two issues related to those raised in this chapter (innate ideas, and rationalism versus empiricism) see the symposium papers in Hook (1969) and Cohen and Wartofsky (1967). Also Blanche (1968), Moravcsik (1967), Katz (1966).

§4: THE PROBLEM OF JUSTIFICATION

4.1. Introduction

Granted that the subject matter of linguistics is competence, how do we obtain information about it? Chomsky discusses this question only within the context of justification. He concurs with the hypothetico-deductive conception of scientific methodology, according to which relatively little of logical significance is involved in the context of discovery - theories or models are to be freely invented, and then tested.

Clearly, the actual data of linguistic performance will provide much evidence for determining the correctness of hypotheses about underlying linguistic structure, along with introspective reports (by the native speaker, or the linguist who has learned the language). (1965: 18)

Given the above sources of data, linguistic theories are to be justified according to criteria of adequacy on three levels - observational, descriptive and explanatory.

What is involved here is a shift in interest from discovery procedures to evaluation procedures. As Sydney Lamb puts it, "criteria for linguistic description [can be] stated simply as specifications which a linguistic description must meet. Criteria of this type are to be used for testing proposed descriptions rather than for prescribing procedures of analysis. Such criteria specify the properties of an acceptable solution" (1966: 6-7). This is perhaps an innocent enough kind of innovation, and it has generally been argued for on the basis of practice in the natural sciences (e.g. Lees 1957).

However the argument has also been reversed. Thus it has been claimed that with the advent of generative grammar linguistics has passed from the "taxonomic", data-gathering, natural history stage of science to the more mature level of deductive theory (Bach 1965). A deductive system is desirable of course for the purpose of deriving testable consequences. The problem in linguistics is that the only deductive system, in any rigorous sense, is a generative grammar itself, and, as Stuart (1969) points out, this deals only with the combinatorial possibilities of selected descriptive features in the primary data. Thus testing of propositions about underlying empirical states of affairs is not provided for in the logical structure of the theory. Stuart goes on to note that attempts to relate grammars to the mental life of speakers are imposed upon this prior logical organization in the context of metatheoretic discussions - the logic within which testable consequences are derived remains quite inexplicit. Given these basic logical difficulties, compounded by an amazing lack of conceptual clarity, talk about hypothetico-deductive systems on a par with those of the natural sciences (no matter how qualified with statements about tentativeness or incompleteness) is simply ridiculous.

As it happens, it is not even necessary to impose these sorts of criteria in order to deal with Chomsky's notion of justification. It doesn't even seem to work on his own terms.

Notice first of all that Chomsky seems to be talking

about evidence on two different levels. Data from performance would be used to test predictions about performance which are implied by the theory of competence. This necessitates assumptions about the relationship between competence and performance, and about a performance model. Introspective reports would seem to constitute a more direct sort of test of propositions about competence, assuming access to the facts of competence through "linguistic intuition".

Secondly, despite Lyons' claim (p.5 above), the problem of "intuition" is of absolutely central importance since upon it depends the significance of the level of descriptive adequacy, which provides justification for both individual grammars and, ultimately, general linguistic theories.

Thirdly, Chomsky himself recognizes difficulties with the use of introspection:

Obviously, every speaker of a language has mastered and internalized a generative grammar that expresses his knowledge of his language. This is not to say that he is aware of the rules of the grammar or even that he can become aware of them, or that his statements about his intuitive knowledge of the language are necessarily accurate. Any interesting generative grammar will be dealing, for the most part, with mental processes that are far beyond the level of actual or potential consciousness; furthermore, it is quite apparent that a speaker's reports and viewpoints about his behavior and his competence may be in error. Thus a generative grammar attempts to specify what the speaker actually knows, not what he may report about his knowledge (1965: 8).

Thus we can say that a native speaker in some sense knows the rules of his language, but he doesn't know what the rules are. Chomsky is admitting that there are two rather severe limitations on the use of introspective data as a source of

evidence: (1) its range is very restricted, particularly with respect to depth; and (2) its accuracy is open to question.

In this chapter I will argue that the kinds of data available from the use of introspection or intuition are inadequate for claiming descriptive adequacy, that considerations of explanatory adequacy do not solve the difficulty, and thus that the problem of justification remains unsolved.

4.2. Justification and adequacy

Chomsky (1965) proposes external and internal grounds for claiming justification of grammars. On the level of descriptive adequacy a grammar is justified "to the extent that it correctly describes its object, namely the linguistic intuition - the tacit competence - of the native speaker" (p. 27). This is external justification, on grounds of "correspondence to linguistic fact".

On the level of explanatory adequacy a grammar can be justified "to the extent that it is a principled descriptively adequate system, in that the linguistic theory with which it is associated selects this grammar over others, given primary linguistic data with which all are compatible" (p. 27). This is internal justification, on grounds of a grammar's relation to a linguistic theory which is part of a theory of language acquisition and thus constitutes an "explanatory hypothesis about the form of language as such". Chomsky notes that because explanatory adequacy is empirically a much deeper level, it is much more rarely attainable than that of descriptive adequacy.

Invoking a condition of generality, Chomsky applies these concepts of adequacy to linguistic theories (as opposed to

individual grammars). We may call a linguistic theory descriptively adequate "if it makes a descriptively adequate grammar available for each natural language" (p. 24). Only to the extent that it meets conditions of descriptive adequacy is a linguistic theory "empirically significant" (p. 34).

In a similar fashion a linguistic theory meets the condition of explanatory adequacy to the extent that it succeeds in selecting a descriptively adequate grammar on the basis of primary linguistic data (p. 25). To this extent the theory offers an explanation for the native speaker's intuition (competence), an account of how it could be acquired. Chomsky observes that such a hypothesis can be readily falsified by demonstrating that it fails to provide a descriptively adequate grammar on the basis of primary linguistic data from some other language (p. 26).

Thus it is clear that, for Chomsky, justification depends on the concept of a "descriptively adequate grammar":

In evaluating a particular generative grammar we ask whether the information that it gives us about a language is correct, that is, whether it describes correctly the linguistic intuition of the speaker ... In evaluating a general theory of linguistic structure ... we ask whether the generative grammars that it selects meet the empirical criterion of correspondence to the speaker's linguistic intuition, in the case of particular languages (1964a: 62).

However the notion is rather unclear, particularly with respect to the question of the "intuition".

4.3. Intuition and adequacy

Peters (1970) distinguishes two senses of "descriptive adequacy". In the first (weak) sense a descriptively adequate

grammar is required to "properly state the linguistic intuitions of a native speaker" (p. 29). The second (strong) sense requires that a descriptively adequate grammar state "not only the correct linguistic intuitions but also the correct generalizations" (p. 41); we then have a "correct account" of the native speaker's competence.

Now Chomsky states that a descriptively adequate grammar is one that "correctly describes the intrinsic competence of the idealized native speaker" (1965: 24). Descriptive adequacy is achieved "when the grammar gives a correct account of the linguistic intuition of the native speaker, and specifies the observed data (in particular) in terms of significant generalizations that express underlying regularities in the grammar" (1964a: 63). He clearly uses the term "descriptive adequacy" in the strong sense.

However, Chomsky also identifies the linguistic intuition of the native speaker as his competence (e.g. 1965: 27), and this would seem to confuse the situation somewhat. It would imply, for example, that a correct description of competence need not necessarily be in terms of linguistically significant generalizations, and this surely makes no sense.

Perhaps this problem can be handled by referring to what Leech (1968: 95) calls the "suppressed ambiguity" in Chomsky's use of the term "intuition". It is applied to both the native speaker's competence, and his ideas about his competence (this latter sense is sometimes signalled by the use of the plural "intuitions") presumably arrived at by introspection.

On this interpretation, when Chomsky states that a grammar achieves descriptive adequacy when it gives "a correct account of the linguistic intuition of the native speaker" (1964: 63), he means that it correctly describes competence - which involves accounting for the native speaker's introspections (intuitions) and going beyond this to statements about the deeper aspects of competence in terms of linguistically significant generalizations. But there are still further ambiguities of "intuition" to be untangled.

Chomsky has referred to a grammar as being "a theory of linguistic intuition" (1965: 19). By this he apparently means nothing more than that a grammar is a theory or model of competence - the native speaker's intrinsic knowledge of his language. The native speaker's intuition is simply his competence. He goes on to say that the "speaker-hearer's linguistic intuition" is the "ultimate standard that determines the accuracy of any proposed grammar, linguistic theory, or operational test ..." (p. 21). This seems to be rather obvious - a grammar is (descriptively) adequate to the extent that it correctly describes its object; namely, the competence or intuition of the native speaker. Notice that the statement, on this interpretation, is vacuous - it leads us directly back to our original question. This shows up more clearly in Chomsky's comments about operational tests:

It is important to bear in mind that when an operational procedure is proposed, it must be tested for adequacy... by measuring it against the standard provided by the tacit knowledge that it attempts to specify and describe (p. 19).

This statement begs the question - how do we obtain information about competence? But our other interpretation of "intuition" is unsatisfactory as well. The native speaker's introspections (intuitions) surely cannot provide any "ultimate standard" given Chomsky's remarks about their reliability.

Of course Chomsky readily admits that raw introspection is not to be depended upon, and in any case does not go deep enough. The native speaker's intuition has to be guided and drawn out by the analyst (p. 24). So the analyst's "intuition" comes into the picture as well. In his statement on the sources of data available, Chomsky allows for introspective reports by the native speaker or by the linguist who has learned the language. Leech here finds another (and worse) confusion about "intuition":

the confusion between analytic insight (a capability of the linguist qua scientific investigator) and introspective consultation of one's own linguistic competence (a capability of the linguist qua native speaker informant).
(1968: 95)

Leech finds many problems with this further ambiguity. I would say that the analyst's intuition (insight) plays a major role even if the linguist does not act as his own informant. (And I tend to agree with Chomsky that this position is "universally adopted in practice" despite methodological discussions to the contrary). It is the linguist qua scientific investigator who decides what constitutes a "substantial and significant class of crucial cases" with reference to the native speaker's intuition, which Chomsky mentions in connection with determination of adequacy (e.g. 1965: 24). It is the linguist who assumes the means of guiding

the native informant in the correct fashion.

This situation is not really so strange. The problem lies not in Chomsky's assuming privileged access to competence on the part of some (transformationally trained) analysts, but in whether or not he can demonstrate independent justification for their hypotheses. If these are the intuitions which are to provide the "ultimate standard" then perhaps we can join Leech in his charge of "unashamed subjectivism" (1968: 87). Certainly Chomsky's rather cryptic remarks about insight versus objectivity (1965: 20) seem to lend force to Leech's assertion, but the situation is not so clear-cut. The argument has to go through another stage yet.

4.4. Justification and evaluation

Consider this statement by Lees (1957: 380-381):

To reject the worse alternative of a pair of proposed theories, the best that the natural scientist can do is propose a so-called 'crucial experiment'; but linguistics may be able to go one step further and formulate rigorous criteria of excellence of grammars.

The natural scientist actually applies a wide range of criteria in assessing theories. Bunge (1967 II: 352-354) lists twenty criteria of several different sorts. One of them is that of depth, the most important result of which is that the set of relevant data becomes more varied. Despite Lees' implication, the setting up of an evaluation procedure in linguistics can hardly be construed as going beyond the natural sciences in rigor, because linguistic theories have not survived attempts at disconfirmation of the psychological claims made for them. In fact these attempts have not been made, and this is the reason for the indeterminacy which the evaluation procedure

is supposed to resolve. Now how does Chomsky develop his position to the point where he feels that he can speak of justification on "deep empirical grounds"?

Stuart (1969) argues that:

the theory only deals with the rationalistic combinatorial properties of the primary data. Thus if we now go on to ask how we can test the correctness of our descriptions, we find ourselves enveloped in a form of circularity. For once the combinatorial possibilities admitted by our initial selection of descriptive features has been established, all we can do is to improve the description rather than to falsify it. Correctness is thus a matter of goodness-of-fit with respect to the primary data, and not a matter of testing hypotheses about empirical states of affairs that underlie these data.

Chomsky tries to avoid this by introducing a new source of data, namely the introspective reports of the native speaker. Predictions about native speaker judgements of ambiguity, paraphrase relations, and the like, can be deduced from examination of the structural descriptions generated by the grammar. The problem is that this source of evidence is under severe limitations. Grammars are underdetermined by the data. So Chomsky goes to the notion of an evaluation procedure, ties it in with the language acquisition device (see Matthews 1967 for a cogent assessment of Chomsky's argument on this point), and starts talking about the need to consider explanatory adequacy in order to achieve descriptive adequacy in any particular case (1965: 41).

All concrete attempts to formulate an empirically adequate linguistic theory certainly leave ample room for mutually inconsistent grammars, all compatible with primary data of any conceivable sort. All such theories therefore require supplementation by an evaluation measure if language acquisition is to be accounted for and selection of specific grammars is to be justified ... (p. 37).

It is clearly the proposed connection between the evaluation measure and the language acquisition device that motivates expressions like "deep explanatory hypotheses". Also, its co-requisite in considerations of explanatory adequacy, additional constraints on the form of grammars, leads to hypotheses about formal universals.

How is an evaluation measure developed?

The major problem in constructing an evaluation measure for grammars is that of determining which generalizations about a language are significant ones; an evaluation measure must be selected in such a way as to favor these. (Chomsky 1965: 42).

Proposals about evaluation metrics (and constraints on grammars) are to be justified in terms of success in providing descriptively adequate grammars. An explanatorily adequate theory is one which provides us with correct descriptions of competence. Notice that in order to set up an evaluation procedure we have to have, or make assumptions about, a descriptively adequate grammar, in order to develop notational conventions on the basis of a sample of correct generalizations. As Peters points out the question of linguistically significant generalizations is one of dealing with "linguists' intuitions about the significance of generalizations not ... linguistic intuitions of native speakers" (1970: 42). Assumptions about descriptively adequate grammars are introduced hypothetically but sooner or later it is necessary to be able to demonstrate descriptive adequacy.

Given the available data we cannot claim strong descriptive adequacy, but weak descriptive adequacy is not sufficient

because it leaves us with the insoluble selection problem (Prideaux 1970). This means we have no basis for claiming justification of general linguistic theories, except the linguist's insight (intuition). Reluctance to admit the need for independent justification (a new kind of data) can only be interpreted as a lack of true concern with depth. Thus, in terms of Chomsky's own stated goals we are led to look elsewhere. And the natural place to look for evidence about psychological claims is psycholinguistics - towards a theory of performance.

4.5. A recapitulation

Chomsky provides for evidence from corpus data and native speaker judgements. These are obviously not adequate for the purpose in themselves. Hence the requirement of generality, which allows for a great deal of systemic corroboration (see e.g. 1965: 41). But given what Chomsky himself says about the range and reliability of introspection, the whole process again becomes a matter of goodness-of-fit, leaving untested the crucial features of the model which are based on assumptions about strong descriptive adequacy, in particular, about what constitutes a "linguistically significant generalization" - assumptions which cannot be grounded on the data available.¹

The inordinate reliance on internal, formal justification did not really seem a problem until it was revealed that the excessive power of transformational rules made it useless (see e.g. Peters 1970). Consider also that Chomsky has said:

Choice among competing theories of language is of course also a fundamental question and should also be settled, insofar as possible, on empirical grounds of descriptive

and explanatory adequacy (1965: 38-39).

So the entire justificatory framework has been undermined.²

Interestingly enough, Chomsky (1965) seemed to anticipate some aspects of this problem and was ready with a further "empirical" constraint:

... the problem is to impose sufficient structure on the schema that defines 'generative grammar' so that relatively few hypotheses will have to be tested by the evaluation measure, given primary linguistic data. We want the hypotheses compatible with fixed data to be 'scattered' in value, so that choice among them can be made relatively easily. This requirement of 'feasibility' is the major empirical constraint on a theory, once the conditions of descriptive and explanatory adequacy are met (pp. 61-62).

The difficulty with this of course lies in the fact that not only is there no empirical basis for the evaluation procedure, but also that any imposed additional structure on the form of grammars - constraints on transformations - needs justification. This sort of proposal constitutes "patching up", or, more correctly in this case, an attempt to postpone serious consideration of how claims of "empirical significance" or "psychological content" can be tested. So it seems that we are forced to look at the question of what it means to say that we have a "correct description" of competence, and this in turn requires us to look at the competence-performance relation and the possibilities of a performance model.

FOOTNOTES

1. (p.42) I have not attempted to attack Chomsky for his use of introspective evidence from the native speaker except insofar as it seems to overstep the limitations he mentions (cf. his additional remarks in MacIntyre 1968). In general, I think that there is probably a legitimate place for this kind of data in the context of Chomsky's kind of justification, and I agree that no useful purpose would be served by objectifying data-gathering procedures for tests of grammaticality and the like. But this requirement (weak descriptive adequacy) is absolutely minimal, rather than ultimate.

... there are many different transformational grammars that generate the same set of structural descriptions. And there are many different sets of structural descriptions that have the same empirical consequences [in terms of predictive power about native speakers' introspective judgements] (Peters 1970: 40).

The problem is that introspection can offer no information about deeper empirical states of affairs, in particular, it cannot be expected to support claims about psychological reality.

The analyst's ideas, insights, intuitions, introspections or whatever, must be confined to the context of discovery insofar as they go beyond what can be expected from the usual native speaker; unless of course Chomsky or someone else is prepared to come up with an empirical theory about introspection.

Finally, I would like to suggest that Leech's charge of "extreme reliance" on the analyst's intuition in transformational metatheory is misleading, in that it draws attention away from what I consider to be the most serious problem - an unusual degree of faith in formal considerations as a guarantor of empirical relevance and of truth.

2. (p.43) For other discussions of these problems see Peters (1970), Prideaux (1970, forthcoming).

Derwing (forthcoming) shows that the situation is generative phonology is parallel to that in syntax. In this case the predicament is the joint product of the lack of constraints on underlying representations and the problem of "naturalness". This of course raises serious doubts about Kiparsky's (1968) attempts to find a "window on competence" by considering diachronic data. The range of evidence is extended over time, but this is still all within the formal domain. In the same way, naturalistic observations of language acquisition do not offer a serious test of propositions about underlying empirical events. Correctness remains still largely a matter of goodness-of-fit.

5.1. Introduction

Obviously any rational person will favor rigorous analysis and careful experiment; but to a considerable degree, I feel, the 'behavioral sciences' are merely mimicking the surface features of the natural sciences; much of their scientific character has been achieved by a restriction of subject matter and a concentration on rather peripheral issues. Such narrowing of focus can be justified if it leads to achievement of real intellectual significance, but in this case, I think it would be very difficult to show that the narrowing of scope has led to deep and significant results ... The experts have the responsibility of making clear the actual limits of their understanding and of the results they have so far achieved ... (Chomsky 1968: v).

I think that this statement of Chomsky's is to some extent correct, as a critical judgement. It is misleading, however, in terms of what it implies about his own work. Expressions like "real intellectual significance" and "deep and significant results" are vague and subjective, but by juxtaposing such remarks with comments about the behavioral sciences vis-a-vis the natural sciences, he gives the impression that we can but interpret such terms in the way in which they are applied to what he calls the "successful sciences" (1965: 20). At this point he usually introduces comments on his own work like "tentative" and "speculative" (see my next chapter for a general attack on this), but he has not actually made any detailed effort at making clear the limiting conditions on his approach and his results. In Popperian terms a theory is "tentative" in the sense that it has not been falsified; that is, it has passed every test that it has been possible to bring to bear on it. And Popper is saying that the theorist should be concerned with falsification at every step in theory construction.

Whatever the merits of this as a description of scientific practice on the part of individual scientists, Chomsky himself speaks of "responsibility" and thus invites evaluation along these lines.¹

In brief, Chomsky's general claim is this:

The general explanatory theory of language and the specific theory of a particular language that results from application of the general theory to data each have psychological content, the first as a hypothesis about innate mental structure, the second as a hypothesis about the tacit knowledge that emerges with exposure to appropriate experience (1966b: 12n).

At the present time, this statement is programmatic; but if the claim (and the program) is to be taken seriously, the model must be made vulnerable - that is, responsible to empirical data that can potentially falsify it. The explanatory power of the general theory rests on its ability to provide correct particular theories. As a hypothesis about "innate mental structure" it is corroborated to the extent that it can (in conjunction with some learning theory) account for the acquisition of "tacit knowledge".

The strongest possible proof of the inadequacy of a linguistic theory is to show that it literally cannot apply to some natural language (Chomsky 1957: 34).

In terms of Chomsky's criteria, this means showing that a theory cannot provide a descriptively adequate grammar for some language, that is, a correct description of the competence or tacit knowledge of a native speaker of that language. Given that the sources of data which Chomsky allows for are inadequate for testing beyond a superficial level, the potential source of falsification is the theory of performance to which the

competence model must be related.

5.2. A caveat

Bever has recently claimed, with respect to studies of grammar as a psychological process:

Linguists defended themselves from the onslaught of inexplicable psychological facts by invoking the distinction between what we know and what we do (1968: 10).

There are three basic versions of the competence-performance relation:

- (a) a competence model is an idealized performance model
- (b) a competence model is somehow to be incorporated into a performance model
- (c) the relation is unspecified, but more indirect than (b)

The third alternative is the one proposed by Fodor and Garrett (1966). It involves the weakest (least falsifiable) claim of the three, and they are led to it by trying to make sense of experimental data while at the same time refusing to consider the competence model (grammar) as invalidated (see below). In doing this they reject the second position, which is that of Chomsky. Chomsky's (1969c) doctrine of notational variants would seem to weaken his claim of deep empirical content, insofar as he refuses to develop a more explicit version of his position with regard to the competence-performance relation. Given this kind of situation, it would seem that any independent support for psychological content (in the sense of failure to invalidate the theory) coming from this direction is hardly to be relied on, because of the refusal either to be specific about testable proposals or to admit falsification when tests are carried out. Yet at the same time practically every

generativist argues for the competence-performance distinction in terms of the first version, by analogy with the study of ideal objects in other sciences. Since it involves the strongest claim, few of them believe in it otherwise.

5.3. The psychological reality problem

Fodor and Garrett (1966) claim that structural descriptions are psychologically real, but that operations (rules) by which these are generated by a grammar are not. The second part of this claim is based on the idea that if a grammar is involved in sentence-processing in a straightforward way (analysis-by-synthesis), then there should be general correspondences between formal features of derivational histories such as length in rules, and performance variables such as ease of recall or processing time. They cite experimental evidence which they claim disconfirms such predictions and conclude that no close relation exists between the psychological process of analysing a sentence and the formal operation of generating it.

Notice that two other alternatives were open to them. First of all they could have denied the validity of the experimental procedures. They do, in fact, discuss this possibility and express some reservations about them. Secondly, they could have claimed that the grammar used was thereby disconfirmed. This they apparently refuse to consider:

Rather, one would best interpret negative data as showing that an acceptable theory of the relation between competence and performance models will have to represent that relation as abstract, the degree of abstractness being proportional to the failure of formal features of derivations to correspond to performance variables (p.152).

L.J. Cohen (in Lyons and Wales 1966: 166-67) suggests that if the grammar could be considered as being at fault in this case, then by altering it one might establish a direct relationship between processes in the grammar and psychological processes. Why do Fodor and Garrett want to save the grammar?

Consider their immediately preceding remarks:

It should be emphasized that, in showing that a predicted complexity order fails to obtain, one has not shown that the grammar is disconfirmed. A grammar is simply an axiomatic representation of an infinite set of structural descriptions, and the internal evidence in favor of the structural descriptions modern grammars generate is so strong that it is difficult to imagine them succumbing to any purely experimental disconfirmation (p.152).

Insofar as one can make sense of this, it seems that grammars are not disconfirmed because they are axiomatizations of sets of structural descriptions, which one cannot imagine being disconfirmed because they are so strongly supported by evidence internal to grammars. From this, one suspects that grammars must remain not disconfirmed in order to support claims about structural descriptions, and this does indeed seem to be what Fodor and Garrett are up to.

In the discussion following their paper, Garrett says:

We take it as obvious that a grammar explicates what is a certain variety of psychological facts - intuitions which speakers of a language have about their language; for example, notions of what is or is not a sentence, notions of inter-sentential and intra-sentential relationships -- in short, a set of complexly-interrelated observations about language structure. These are just the facts the linguist has demanded that the structural descriptions produced by his grammar should mark. In this sense, it is undeniable that, to the extent the observation statements are sound, and to the extent the linguist has been successful in writing his grammar properly, the structural descriptions produced by the grammar are 'psychologically real' (Lyons and Wales 1966: 175, emphasis mine).

It certainly does not follow from this that the perceptual routine assigns a structural description to a wave-form, so we are left wondering whether this is the only basis for claiming psychological reality of structural descriptions.

At this point consider the implications of the demonstration of the very great power of transformational rules. There would be no point in wanting not to disconfirm a grammar on the basis of lack of correspondences of the type discussed above, since there can be many different transformational grammars that generate the same set of structural descriptions (Peters 1970). Garrett's formulation of "the psycholinguistic problem":

A search for an analysis of the connection between the way the structural description is specified by the grammar and the way it is 'specified' by speakers and hearers during the production or understanding of a sentence (loc. cit.),

becomes strange (if it is granted that it made sense in the first place), if the indirect nature of the relationship between competence and performance is insisted on because of the failure of formal features of derivations to lead to correct predictions about performance parameters. The obvious thing to do would be to alter the grammar in whatever ways necessary to improve its predictive power. But at the same time the whole issue is placed in doubt because there can be many different sets of structural descriptions which account for the native speaker's intuitions. So the basis for claiming psychological reality of structural descriptions on grounds of strong internal evidence from the grammar is lost as well, not that it was acceptable in the first place if

"psychological reality" is taken to mean active deployment in perception.

The incoherence of the Fodor and Garrett position indicates some rather fundamental considerations have been overlooked. I have already mentioned the possibility that equating competence with some kind of knowledge on the part of the native speaker may be misleading (3.2 above, see also Schwartz 1969); and I would now like to suggest that the term "competence" itself is misleading. Of course Chomsky is free to define terms in whatever way he wishes provided it is made clear that this is what he is doing and, more importantly, that he use his own terminology consistently. Chomsky is careless in the second respect. For example, there has been a lot of confusion over the term "generate" and Chomsky considers the possibility of discarding it (1965: 9). He decides against this, insisting on its appropriateness, and then proceeds to misuse it himself:

The notion 'kernel sentence' has, I think, an important intuitive significance, but since kernel sentences play no distinctive role in generation or interpretation of sentences, I shall say nothing more about them here. One must be careful not to confuse kernel sentences with the basic strings that underlie them. The basic strings and base Phrase-markers do, it seems, play a distinctive role in language use (1965: 18, emphasis mine).

5.4. Competence, mechanisms, and performance

The subject matter of linguistics is competence - a mechanism underlying actual behavior (performance). N.S. Sutherland conceives the task of psycholinguistics as "by doing experiments, to find out what are the mechanisms which underly linguistic competence" (in Lyons and Wales 1966: 158). Now in

Chomsky's terms the mechanism underlying competence is the language acquisition device to which he assigns innate structure. This is one of the things Sutherland is talking about. The other is the mechanism responsible for language performance in adults. In other words, for Sutherland the speaker-hearer mechanism has or displays competence, but its competence is not another mechanism which is somehow attached to or incorporated in the first - this would be committing a category mistake. To fully explain behavior involves working out what the mechanism is and what it can do. To explain the competence of a device would be to show how it does what it can do.

... the technical term 'competence' refers to the ability of the idealized speaker-hearer to associate sounds and meanings strictly in accordance with the rules of his language (Chomsky: 1967a: 398).

To describe an ability one could use a set of adjectives ("fantastic"), list a set of possible achievements, or perhaps say something about how it works:

This competence can be represented, to an as yet undetermined extent, as a system of rules that we can call the grammar of his language (Chomsky 1964a: 51).

By a 'generative grammar' I mean a description of the tacit competence of the speaker-hearer that underlies his actual performance in production and perception (understanding) of speech. A generative grammar, ideally, specifies a pairing of phonetic and semantic representations over an infinite range; it thus constitutes a hypothesis as to how the speaker-hearer interprets utterances, abstracting away from many factors that interweave with tacit competence to determine actual performance (Chomsky 1966a: 75n).

Now this sounds like Chomsky's "competence model" is a model, under conditions of idealization, for the speaker-hearer mechanism. Given what Sydney Lamb calls a "dynamic

interpretation" it would seem possible to test it as a "hypothesis as to how the speaker-hearer interprets utterances".

Chomsky, however, specifically denies this kind of interpretation. He has said, when speaking of finite automata, "The assumption that sentences are produced or recognized by a device of this kind tells us almost nothing about the method of processing" (1964b: 124n), hence seeming to imply that a generative grammar does. And, more strongly, he speaks of grammars providing the devices that are employed in the production and understanding of speech (1964b: 127n). But he specifically denies, for example, that the speaker produces a P-marker of a sentence from the top down (1964b: 126), and recently denied that general directionality between the components of a grammar is any more than "suggestive" with respect to a performance device (1969c).

From this it would seem that Fodor and Garrett's problem was of their own making, as a result of a misinterpretation of Chomsky. Yet the position they were arguing against:

... that a performance model ought to consist of a model of linguistic competence (a grammar) plus some further component or components at present unknown; jointly these components are somehow to issue in the utterance or understanding of sentences (1966: 138),

seems clearly to be that of Chomsky.

No doubt, a reasonable model of language use will incorporate, as a basic component, the generative grammar that expresses the speaker-hearer's knowledge of his language (1965: 9).

(Notice once again that he speaks of a knowledge or competence sub-mechanism or component as being of the same logical status as the other components). But the difference becomes clear

when the performance factors which Chomsky allows for are specified.

Steinberg (1970a: 180) presents Chomsky's position in this way:

Such accomplishments as the uttering of a sentence are conceived to be the results of an interaction between the knowledge we have of our language and various application factors such as 1) memory limitations, 2) distractions, 3) shifts of attention and interest, 4) errors, 5) beliefs, and 6) rules for competence use.

The first five of these application factors would seem to indicate that the competence-performance distinction is one between an idealization and its realization, but the sixth one shows that this interpretation is misleading.

... although we may describe the grammar G as a system of processes and rules that apply in a certain order to relate sound and meaning, we are not entitled to take this as a description of the successive acts of a performance model ..., in fact, it would be quite absurd to do so ... If these simple distinctions are overlooked, great confusion must result (Chomsky 1967a: 399).

It would not be absurd, and it would be far less confusing to try and write grammars which could be interpreted in this way - at least we could be sure of when they are wrong.

Steinberg argues that Chomsky's competence component is theoretically superfluous on grounds that:

Chomsky's governing use rules have the effect of simply reordering the rules of competence so that the processes of production and understanding may be performed (1970a: 187).

As such they amount to proposals for alternative grammars.

This leads back to the question asked of Fodor and Garrett:

why not simply alter the grammar so that it leads to successful predictions? Such a refusal to assign a sacrosanct status

to the competence grammar has a further value, in that it frees one from the tyranny of the assumption that a performance grammar or model must somehow be like what Chomsky's formal theory suggests it is.

Chomsky's recent statements are very revealing in this respect. He can see no way to distinguish empirically between what appear to be two very different alternative grammars (1969c). I suspect that this is either because they are both semantically abstract - incapable of interpretation - and thus have no "psychological content", or because Chomsky is unwilling to admit that his own prophecy has come true:

Perhaps the day will come when the kinds of data we now can obtain in abundance will be insufficient to resolve deeper questions concerning the structure of language (1965: 21).

But he has come up with another cryptic statement to keep us guessing: namely that there is a difference in logical character between competence and performance and that to confuse the two kinds of account would be a category mistake (1969c: 7). One can only wonder how they can both be included in the same empirical theory, or why they should be.

Bloomfield was worried about linguistics being undermined by continual shifts in psychological doctrines of the mentalistic sort. Many psychologists who are investigating language are now feeling threatened by constant reformulations in formal linguistics. Jakobovits, for example, speaks of the "crisis in psycholinguistics" resulting from the "excessive dependence of psycholinguistics on linguistic theories" (1969: 156). Foundational considerations preclude a completely autonomous

science of language - an empirical basis is necessary; so if linguistics and psychology must be related in some way, what must the character of that relationship be? The answer to this question lies in examining the factors underlying the problems of interpretation that we are faced with at the present time. Since the major difficulty is that of relating the linguistic theory of competence to the psychological theory of performance, it would seem a reasonable suggestion to explore the possibility of constructing an explicit psycholinguistic theory, with formal linguistic variables and psychological variables being embedded in a single logical and conceptual apparatus. And I also suggest that it would be unreasonable to continue in Chomsky's direction in the hope that his "tentative" theory will prove to be as deep and successful as he would have us believe.²

FOOTNOTES

1. (p.44) It is interesting that Chomsky dismisses the question as to whether linguistics is a science as terminological (1965: 20), while Katz insists that linguistics is a science. On the other hand, Katz regards the theory of language as falling within the province of epistemology (1966: 118), while Chomsky has said: "Linguistics is really a theoretical biology, if you like, a theoretical psychology" (quoted in Jakobovits 1969). The point is that if Chomsky's warrant for his deep claims is different from those in terms of which I am judging him, then he should stop these misleading attempts at status-by-label, and state his position clearly. Barring that, there is no choice but to apply criteria based on standards of practice in cognitive psychology and other disciplines which make use of postulated unobservable mechanisms and entities.
2. (p.56) For additional discussion on the general issues raised in this chapter see Schlesinger (1967). Steinberg (1970b) offers an attack on the doctrine of notational variants.

§6: AN INDEFENSIBLE DEFENSE

I have sketched tentative outlines of some components of a prospective systematic theory that does not yet exist.

- - R.D. Laing (1969)

Chomsky has conceded that "sooner or later it is going to be necessary to discover conditions on theory construction, coming presumably from experimental psychology or from neurology, which will resolve the alternatives that can be arrived at by the kind of speculative theory constructions linguists can do on the basis of the data available to them" (in Darley 1967: 100). Furthermore, he has admitted that "linguistic work, at what I believe to be its best, lacks many of the features of the behavioral sciences. Nor is it obvious that the development of explanatory theories in linguistics merits the honorific designation 'scientific'" (1969a: 56).

However, he has also said, "The arguments in favor of this informal procedure seem to me quite compelling; basically they turn on the realization that for the theoretical problems that seem most critical today, it is not at all difficult to obtain a mass of crucial data without use of such techniques" (loc. cit.). Are these critical problems deep, and hence our solving theories deep? "As in any other field, the important problem in linguistics is to discover a complex of data that differentiates between conflicting conceptions of linguistic structure in that one of these conflicting theories can describe these data only by ad hoc means whereas the other can explain it on the basis of some empirical assumptions

about the form of language" (1965: 26). I have been arguing that it is just for lack of the right kind of data (and the right kind of theory) that Chomsky fails, in particular because he is not willing to recognize that his approach has built-in limitations of the kind that he at best is only willing to admit as necessary to overcome "sooner or later".

Charles Hockett (1948) drew an analogy between the analytical procedure of the linguist and happenings in the nervous system of a child learning his first language. The end-product for the child is a state of affairs in his nervous system, for the linguist a set of statements (a grammar). According to Hockett we are justified in relating our set of statements to the child's state of affairs when we can make successful predictions beyond the original corpus. It is at this point that linguistics becomes a science (God's truth) rather than a game (hocus-pocus).¹ A few years later (1954), Hockett set out a specific list of criteria which models for grammatical description must meet in order to be acceptable. Hockett says that given two models that meet his requirements, we must conclude that they differ only stylistically. Crudely put, what Chomsky does is introduce a new formalization, reformulate Hockett's criteria (adding the one about satisfying the native speaker's intuition), and point out the need for an evaluation procedure. This development by Chomsky ends with the notion of explanatory adequacy, which attempts to provide foundations for the evaluation procedure. Thus the analogy between the linguist and the child used by Hockett is continued further, leading to the hypothesis

that the child prefers certain rules or that a certain kind of simplicity is innately favored - what is claimed to be linguistically significant is somehow naturally so. But what possible claims can we make about a genotypic neural capacity, when we must still allow very different theories as being only notational variants? They can be regarded as notational variants only if they are semantically as well as epistemologically abstract. Chomsky has admitted that there is a problem with the empirical interpretation of grammars (1968: 85). But he does not seem to take it seriously. This is not the central problem, which is that of finding a theory for the data available. But there are data available which could allow us to have more faith in the theory if it could be clearly stated, in other words if we admit the need for it and get back to rigorous theory.

Now suppose Chomsky had made a declaration like that of Laing's above. Then his ultimate aim is a systematic theory which is explanatorily adequate in the sense that it is based on empirical as opposed to ad hoc assumptions. This is adequacy-in-principle. Only then can we seriously raise the question of feasibility (1965: 55). Now a model which is adequate-in-principle and feasible still only meets the criterion of sufficiency. And the problem of deciding between two sufficient simulations, for example, is no different from the general problem of deciding between two competing theories (Fodor 1968a). This kind of ultimate non-vicious circularity (the answers you get depend on the questions you ask) is intrinsic in the scientific or any rational endeavor,

so what's the hassle? After all we are only at the stage of a "tentative first approximation" and Chomsky himself has warned against premature orthodoxy.

Perhaps it is when faced with this kind of argument that the point is reached where the critic turns from demonstration to persuasion. But what develops if I borrow some of Chomsky's own debating tactics, used in his attacks on taxonomic linguistics and empiricist accounts of language-learning? His most successful technique has been that of taking his opponents publicly-stated principles, making them precise if necessary, and pushing them to unacceptable conclusions. My use of this technique against him is aimed at forcing the issue to the point where he must either repudiate his own statements or deny the legitimacy of his own kind of argument. In other words I reach a conclusion like this:

A careful analysis has shown that insofar as the system of concepts and principles advanced can be made precise, it can be demonstrated to be inadequate in a fundamental way. (Chomsky 1968:3)

This fundamental inadequacy lies in the informal procedure which Chomsky thinks can be so compellingly argued for.

The argument goes like this. The defense resting on the fact that certain intellectual horizons are remote beyond the scope of present understanding and perhaps ultimately so, is not acceptable in this case. Recall that translucent boxes are really collections of smaller black boxes. But there are two levels of investigation here. Depth is gained by hypothesizing about the workings of the big box (experimental cognitive psychology) or by opening the big box (neurology).

Let's assume that Katz (1964) is right when he says "even if the linguist could look inside a speaker's head he would be unable to figure out the structure of the mechanism from the electrochemical events going on there" and thus that we can dismiss neurology as "talk about synapses, nerve fibers and such". Right away the defense in terms of ultimate limitations loses force. (We haven't met the sufficiency criterion which can be put forth on this level.) We are left with that based on the limits of present understanding and technique - here Katz talks vaguely about compatibility or isomorphism, while Chomsky mentions constraints on theory-construction ("sooner or later"). This doesn't work either because presently-available techniques of experimental psychology can be used in deciding between alternatives provided they are well-specified and truly based on empirical assumptions. In fact, despite all his talk about linguistics and cognitive psychology, Chomsky doesn't see them inter-relating in any significant fashion because psychology refuses to "try to do what every other science does, namely, to try to find some underlying theories that will account for behavior" (in Darley: 100). Such a statement is either based on ignorance, or constitutes a deliberate attempt at evasion. (The statement was made in 1965, by which time the new trend in psychology was no longer new).

Now presumably by admitting that linguistic explanation is not scientific Chomsky is emphasizing that it is the task of linguists to propose alternatives and that our kinds of data are adequate for this. Indeed he at times seems to imply

that if we can arrive at the point where we have an explanatory theory which is based on empirical assumptions (that is, not ad hoc - "the important problem in linguistics") then we can accept it as being the right one. But recall his contention is that "choice among competing theories of language is of course a fundamental question and should also be settled, insofar as possible, on empirical grounds of descriptive and explanatory adequacy" (1965:38). Yet he also anticipates that "there will come a point, no doubt, and I think in some areas of linguistics it may already have been reached, where one can set up alternative systems to explain quite a wide range of phenomena. One can think that this or that system is more elegant and much more deep than some other, but is it right?" (in Darley 1967: 100).² Chomsky thinks this might already be the case in phonology, and I have shown that there is a parallel problem in syntax (see also Prideaux 1970, and Derwing forthcoming).

Now in this thesis I argue that we need to consider performance in order to claim adequacy in principle let alone feasibility, and that these same considerations provide a way of deciding between genuine alternatives. In fact Chomsky's problem of deciding between different theories is something of a phantom problem, because the range of possible tests is extended in the process of constructing a theory, and made use of. If this is not so then talk about empirical adequacy, or about insight for that matter, can hardly be taken seriously.³ It is interesting that Chomsky seems to indicate the question of whether linguistics is a science is

somewhat uninteresting because it is a terminological issue, while Leech (1968: 87) has managed to somehow get the impression that the philosophy of science closest to that of Chomsky and cohorts is that of logical positivism. In any case I am concerned to show that Chomsky's work is neither conceptually nor logically coherent in terms of his own stated aims and beyond this, insofar as it can be clearly stated, is seriously misguided in terms of methodology.

A defense based on the theory's tentative and partial character is presumably based on the conviction that we suspend judgement for the time-being. This would be quite acceptable if there was any significant effort being directed at anything but the theory's formalisms, and if there was any sign that the approach justifies such faith. As far as I am concerned the theory is unjustifiable in any serious sense, and insofar as those attempts which have been made are concerned, they raise serious doubts about the viability of the entire framework.

In fact the defence often seems to rest on taking very literally the idea of the hypothetico-deductive method. As things stand now both the "hypothetico" and the "deductive" sides are advanced for consideration but the link between them is somewhat tenuous to say the least. Chomsky at times, and his followers usually, give the impression that their approach is valuable because they, unlike most behavioral scientists, go beyond mimicking the surface features of the natural sciences, mainly in not refusing to theorize about unobservables and in their disdain for inductive methods.

As I have shown, they are correct in this as far as they go. Chomsky has said many times that, in effect, data are cheap, and what is needed is a theory to account for them. But as I have also shown the kinds of data available (in terms of his methods) are restricted in a crucial way - that is, in a way crucial to Chomsky's own goals. Stuart (1969) has remarked on the triviality of prediction in linguistics, while others have commented on the fact that the theory's predictive power greatly outweighs its explanatory power. I submit that this situation is the result of superficial mimicking of the deep features of the natural sciences. Interestingly enough, the hypothetico-deductive reconstruction itself is now increasingly under attack (see e.g. Salmon 1966). And in terms of those psychologists who simply accepted their task as one of demonstrating the psychological validity of Chomsky's model, some of them seem to be developing serious misgivings about the enterprise (e.g. Bever 1968). The point is that there is something very questionable about constructing an elegant formal algebraic theory and then searching for a psychological model (with appropriate quantifiable variables) to tack on as confirmation. And we are faced with this situation precisely because Chomsky has not really foregone the Bloomfieldian dichotomy, and is still playing it safe.

Stuart (1969) has also pointed out the ambivalence of our attitude towards linguistics vis-a-vis other disciplines impinging on the problems of language, and I think this explains the problem with Chomsky. He is making deep representational claims while at the same time refusing to develop any specific

theory containing those claims and implying that linguistics is not properly concerned with the kinds of data necessary to justify those claims. Linguists are to describe competence and to account for its acquisition, psycholinguistics is to show how all this is realized in performance. My position is that if linguistics must study competence in order to be a serious discipline, then it must also investigate the ways of justifying claims about competence as well if it is to be taken seriously. We can afford to play it safe only at the risk of becoming uninteresting despite our delusions about ourselves.

FOOTNOTES

1. (p.59) In the footnote to this article Hockett says:
"Preston [to whom he is replying] might reject reference to the nervous system of a speaker as he rejects de Goeje's reference to the 'mind' of the speaker. For our purpose predictability is the factor of importance; it would not matter whether the internal state which correlates with speech were in the central nervous system, the soul, or the small intestine. But when we speak as scientists, we have the right and duty to accept the findings of other fields of science: psychology refers such matters to the nervous system, not to any other physical or non-physical component of a human being." Given a psychology which uses a more abstract "mentalist" level of conceptualization, the parallel between Chomsky and Hockett becomes more complete, and Chomsky's position becomes the more credible.
2. (p.63) This latter statement was also made in 1965, but not to linguists.
3. (p.63) Consider Leech's remarks on Chomsky's position concerning insight versus objectivity. "In this connection, a rather novel philosophy of science is advanced: that the primary purpose of science is not to explain and predict observable happenings, but to 'gain insight'." (1968: 90). I hardly need to point out that this is an incredible distortion of Chomsky's position. But Chomsky does say: "a good case can be made for the view that the natural sciences have, by and large, sought objectivity primarily insofar as it is a tool for gaining insight (for providing phenomena that can suggest or test deeper explanatory hypotheses)." (1965: 20). Thus by his own admission, part of the search for insight is the search for tests of hypotheses. In fact objectivity (testability) is the touchstone for claiming insight.

§7: CONCLUSIONS

Examination of generative grammatical theory leads to a primary conclusion of pervasive obscurity - conceptual vagueness and ambiguity, and logical inscrutability. Hence, it is reasonable to say that Chomskyan grammars have no specific interpretation in any significant empirical space, and, therefore, that claims about psychological content and about explanation are vitiated. Similarities between the generative approach and the hypothetico-deductive method seem to be either superficial or illusory - we have no deep theory. These conclusions can be regarded as a demand for explication and reconstruction.

More positively, Chomsky has reintroduced the kinds of goals that linguistics should never have lost sight of. While his theory's usefulness in dealing with these new domains is severely constrained, we can perhaps learn from its inadequacies. In particular, we can overcome the schizophrenic character of our efforts by avoiding the combination of rigorous but empty formalisms with a haphazard or uncritical approach to basic methodological issues.

In this regard it is easy to say that linguistics should evolve in the direction of greater biological realism and a central role for experimental investigation. The purpose of foundational investigations is to show how we are to understand such a directive.

EPILOGUE

Chomsky has lost some of his early credibility by taking on a rather blurred outline as opposed to the clear, hard rigor of his first work. His prefatory remarks to Syntactic Structures have perhaps been forgotten to the extent that they have threatened to become prophetic. Insofar as there has been any rigorous theory I have criticized it - but doing so in terms of my dissatisfaction with his answers to questions I probably would not ask, as a linguist, if not for his remarkable impact.

Chomsky's rise to election as a "Modern Master" can be accounted for, in what seems to me to be a reasonable fashion, by considering the historical conditions in which it took place.¹ Chomsky seems to have filled a vacuum in two ways, or, more correctly, filled two vacuums. Bloomfieldian linguistics lacked a deep theory; Chomsky has attempted to provide one. But there was another reason, besides methodological bias, for the "theory vacuum". The descriptivist project, a necessary preliminary, lacked an essential ingredient - there was a "syntax vacuum". When Chomsky, in an absolutely brilliant fashion, filled this gap, it made sense, for the first time, to inquire as to what deeper meanings could be attached to the intellectual products of linguistic research. The fact that the syntactic theory put forth was based on the concept of a "generative grammar" made such speculation inevitable and determined its future course. Thus,

in terms of historical sequence, we have an inversion of the hypothetico-deductive approach and the question is raised as to how grammatical theory works as a psychological theory. Chomsky's formal linguistic theory, while solving a great many problems inherited from the descriptivist era, has led to new problems which it cannot handle, because it is a formal theory. The new problem-structure must be psycholinguistics, rather than continue to merely make dubious claims; but this realization could not have come about unless those claims had been made. There has been no "retrogression".

FOOTNOTES

1. (p.69) This final assessment has grown out of a series of conversations with Dr. Gary D. Prideaux. In particular, the notion of a "syntax vacuum", and its role in these developments, is his.

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